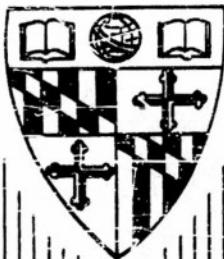
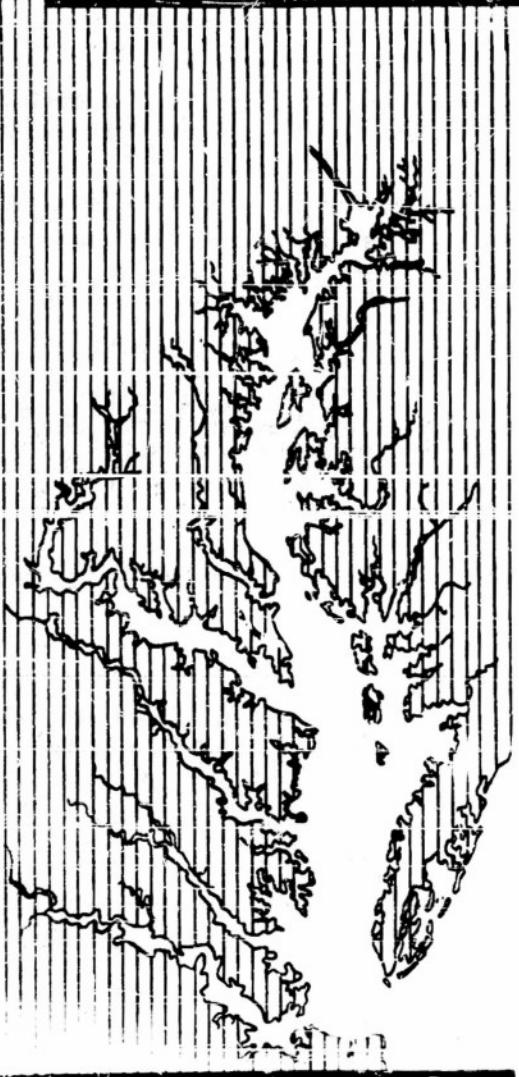


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THE CHESAPEAKE BAY INSTITUTE of The Johns Hopkins University



NOTES FROM THE CONFERENCE
ON THE THERMOCLINE OF
25-27 MAY 1953

Reference 54-2
March 1954

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CHESAPEAKE BAY INSTITUTE
THE JOHNS HOPKINS UNIVERSITY

NOTES FROM THE CONFERENCE ON THE
THERMOCLINE OF 25-27 MAY 1953

Edited by
M. J. Pollak

This report contains results of work carried out for the Office of Naval Research of the Department of the Navy under Research Project NR 082-108, Contract Nonr 248(39), and constitutes the final report under the contract.

Reference 54-2
March 1954

D. W. Fritchard
Director

INTRODUCTION

In March 1953 the Chesapeake Bay Institute of the Johns Hopkins University submitted a proposal for Office of Naval Research support of a conference on the thermocline. The following is quoted from the proposal.

"Any attempt to develop techniques of predicting SONAR ranges at sea must involve the forecast of the thermal conditions. The major feature of the thermal structure is the thermocline. Despite the fact that observations of the thermal structure are the most numerous of any oceanographic measurement, our actual knowledge of the origin and behavior of the thermocline is woefully inadequate.

"Recently a number of highly qualified oceanographers have suggested that a symposium on the thermocline would now be of considerable value in helping to outline the directions that research in this field should take."

That same month the proposal was accepted by the Office of Naval Research and a Task Order was established under Contract Number Nonr-248(39), as project NR 082-108. The Scientific Officer under this Task Order was the Head, Geophysics Branch, Earth Sciences Division, Office of Naval Research. Under the terms of the contract the Chesapeake Bay Institute provided for the meeting place for the conference, as well as for the cost of accommodations, subsistence and transportation of the non-government employed participants.

The subsequent Conference on the Thermocline was held from 25 to 27 May 1953 at Big Meadows Lodge on the Skyline Drive in Shenandoah National Park, Virginia. The following participated in the conference:

Dr. G. C. Ewing } **Mr. J. D. Cochrane** } Scripps Institution of Oceanography

Mr. Henry Stommel	}	Woods Hole Oceanographic Institution
Mr. F. C. Fuglister		New York University
Dr. W. V. R. Malkus	}	Brown University
Dr. Bernhard Haurwitz		Institute of Advanced Study
Dr. Gerhard Neumann	}	Texas A&M College
Dr. R. B. Montgomery		Massachusetts Institute of Technology
Dr. Carl Eckart	}	Chesapeake Bay Institute
Dr. J. C. Freeman, Jr.		Naval Weather Service, Canada
Dr. T. F. Malone	}	Operations Evaluation Group (MIT), Opnav
Dr. D. W. Pritchard		Navy Electronic Laboratory
Mr. M. J. Polak	}	Naval Research Laboratory
Mr. W. F. Ganong		Hydrographic Office
Mr. Carl Behrens	}	Bureau of Ships
Mr. E. R. Anderson		Bureau of Aeronautics Project AROWA
Mr. R. J. Urick	}	Office of the Chief of Naval Operations
Mr. J. J. Schule, Jr.		}
Mr. B. K. Couper		
LCDR D. R. Jones, USN		
CDR D. F. Rex, USN		
Mr. G. G. Lill		
LTJG J. A. Knauss, USNR		
Mr. James Hughes		
Mr. R. C. Vetter		

A number of others who had been invited were unable to attend. Dr. G. C. Ewing very kindly consented to act as moderator, and the orderly trend of the discussion was due, in large part, to his conduct of the meetings.

In order to achieve a high degree of informality at the conference and the freest possible exchange of ideas, no formal papers were invited. The only exception to this plan was a pre-conference invitation to Dr. R. B. Montgomery to present a summary of the existing knowledge of the origin and behavior of the thermocline, as a starting point for the discussion.

In keeping with the informal nature of the conference, it was planned not to record the discussion or to produce any formal minutes of the proceedings. However, just prior to the meeting, it was decided to attempt to obtain a record of the discussion by means of a wire-recorder then

available at the Chesapeake Bay Institute. This recording equipment turned out to be somewhat inadequate for the job and the brevity and incompleteness of the transcribed discussion is partly attributable to this cause.

Large portions of the wire-recording lacked sufficient clarity to permit of a coherent transcription. Other portions could not be used because they were meaningless without the diagrams and equations which had been sketched on the blackboard in impromptu fashion. Although Mr. B. K. Couper generously put his very copious notes of the conference at our disposal, these did not suffice to overcome the basic shortcomings of the recording.

Fortunately, the quality of the wire-recording during most of the summarizing remarks was considerably improved. As a result, the transcribed discussion consists primarily of the comments made at the final session of the conference.

In view of the above mentioned difficulties, it is hoped that the omission of many pertinent remarks made during the conference and the complete absence of statements by some of the participants will be regarded with tolerance by those who may find themselves wronged in such manner. No slight or censorship was intended.

The editing of the transcribed comments has been kept to a minimum, consisting largely of deleting redundancies and incomplete sentences and regrouping some clauses for the sake of clarity. It should be kept in mind that these comments were made extemporaneously, unless otherwise noted.

and hence lack the polish expected of a written paper. In view of the time which had elapsed since the conference took place, it was not deemed practical to submit the notes to their respective authors for correction or revision. The editor therefore accepts the responsibility for any misinterpretations of the original remarks which may appear in their present rendition.

CONFERENCE NOTES

R. B. MONTGOMERY

(Manuscript with post-conference revisions, dated 12 June 1953)

Summary of some of the existing knowledge of the origin and behavior of the thermocline

(Preparation of this summary was supported in part by the Office of Naval Research through contract with Brown University.)

1. History

The word thermocline was introduced by the American limnologist E. A. Birge in 1897 (Birge, 1897, pp. 287, 295; 1904). He regarded the word thermocline as synonymous with the German word Sprungschicht, which had come into use previously. The English sometimes use the term discontinuity layer.

Birge used the word thermocline to designate a water layer in which the temperature decrease with depth is greater than in the overlying and underlying water. According to this definition, a thermocline can occur only at intermediate depths, never next to the surface or to the bottom. Birge ascribed the discovery of the phenomenon to Simony fifty years earlier in Germany.

It is noteworthy that Birge discusses two ways in which a thermocline and the overlying nearly isothermal surface layer can form. One is by surface warming followed by surface cooling. The other, which he was apparently the first to appreciate and which he considered more important,

is by surface warming accompanied by wind stirring of the surface layer.

In fresh-water lakes, the density distribution is determined by the temperature distribution alone, so thermocline and Sprungschicht are fully synonymous and clearly defined. In salt lakes and in the ocean, however, one might think of Sprungschicht as representing any layer more stable than the water above or below. The meanings of thermocline and Sprungschicht become uncertain unless defined more specifically, and they are not necessarily synonymous with each other or with stable layer.

In the open ocean, the effect of salinity on density and stability is usually minor, so by the word thermocline we usually mean a layer that both is more stable and has greater temperature decrease with depth than the water above and below. I shall not attempt to define the word thermocline in general. Often the primary consideration is the density distribution rather than the temperature distribution, so perhaps the word thermocline is not the best one to designate what we are discussing at this conference. Internal stable layer is a possibility. (At the conference, Fuglister used the word halocline and pycnocline. Perhaps the latter best describes the subject of the conference.)

2. Classification

In the ocean three types of thermocline can be distinguished: The main thermocline or permanent thermocline, which is a normal feature of the open ocean except in high latitudes. The seasonal thermocline, which appears in spring and disappears in fall, and which is a normal feature of the open ocean except in low latitudes. The diurnal thermocline,

which appears in the morning and disappears in the evening, conditions permitting.

Limnologists have only the seasonal and diurnal thermocline to deal with. The thermocline in limnology is the seasonal thermocline, while in oceanography it is more apt to be the permanent thermocline. The permanent thermocline owes its existence to the fact that the surface water, a part of the local climate, never gets so cold as the deep water, which has come from higher latitudes, or at least from colder climate. Lakes are not large enough to be subject to different climates; the deep water is essentially formed locally in the season when the surface water reaches maximum density.

In addition to the three types named above, there are the thermoclines and stable layers that occur in coastal waters, estuaries, and where there is melting ice or heavy precipitation and that are associated with salinity contrasts adequate to produce an internal stable layer.

3. Scope

Of course the thermocline cannot form an isolated topic for study. For example, the overlying layer that is relatively homogeneous is perhaps the more basic phenomenon; the thermocline might be regarded as a by-product. As another example, the thermocline is greatly distorted by currents and by storms. Indeed, hardly any phase of so-called physical oceanography is of negligible significance in connection with the thermocline.

It is important to note also the fact that inversions in the atmosphere are the counterpart of thermoclines in the ocean and lakes. Sloping

inversions are called frontal surfaces, as are sloping thermoclines. The intersection of atmospheric or oceanic frontal surfaces with boundaries of the medium are called fronts. The importance of atmospheric fronts is common knowledge, yet there is no adequate explanation of their genesis. Some text books discuss frontogenesis, treating it as a result of deformation in the field of mean velocity. To me, deformation seems utterly inadequate as a process to convert the very small gradients of insolation into the large gradients found in inversions, thermoclines, and frontal surfaces. (Of course deformation is important in the maintenance of fronts after genesis.)

It seems to me that meteorologists lack an understanding of the basic process responsible for frontal surfaces. Oceanographers are perhaps ahead, because in a hazy, qualitative way they better realize the importance of mixing processes. At any rate, knowledge gained about one sort of internal stable layer can be helpful in understanding the others.

The effect of the thermal and saline structure of the ocean on sound propagation is comparable in several respects with the effect of the thermal and humid structure of the atmosphere on short-wave radio propagation. A book on the latter subject has been edited by Kerr (1951).

4. Genesis

When the sun warms water in the absence of wind, the resulting temperature gradient is greatest at the surface. In the presence of wind, however, the maximum gradient is found submerged as a thermocline;

this is indeed a remarkable phenomenon. I do not believe that this phenomenon would be predicted by any person ignorant of its natural occurrence, no matter how familiar he might be with laboratory fluid behavior. (Perhaps engineers dealing with mechanical mixing of liquids like paints have noted comparable phenomena.)

Having observed the natural phenomenon, we see that the wind produces stirring in the water, and that this stirring does not extend very deep. The stirring thoroughly mixes a surface layer and makes it isothermal. Stirring thereby creates the thermocline.

There is evidence that the thickness of the isothermal surface layer depends in part on the wind speed. There is also evidence that the thickness depends on latitude. Ekman (1905) showed that drift currents, because of the earth's rotation, do not extend to the bottom but are limited to a relatively thin surface layer, the thickness decreasing with increasing latitude. It is plausible that the stirred layer coincides with the drift-current layer.

Rossby and Montgomery (1935) made an empirical study of the thickness of the isothermal surface layer h_w in relation to the wind speed v_a at a height of some 10 m and to latitude ϕ and found that

$$(h_w/v_a) \sin \phi = 2.36 s.$$

The effect of latitude seems connected with the earth's rotation, so it is reasonable to introduce the Coriolis parameter

$$1 = 2 \Omega \sin \phi$$

and form the nondimensional number

$$h_w l / v_a = 3.47 \times 10^{-4}$$

This study was based on serial temperatures by reversing thermometer from several expeditions and on a few records made with the 'oceanograph', precursor of the bathythermograph. No similar study has been made of the extensive material subsequently collected by bathythermograph.

The significance of this result is enhanced by the fact that a quantitatively comparable empirical result was found for the thickness of the atmospheric homogeneous layer, h_a , at Boston,

$$h_a / v_a = 136 \text{ s},$$

or, as the latitude is $42^{\circ}22'N$,

$$h_a l / v_a = 0.0134.$$

The two nondimensional numbers that are comparable are $h_w l / v_{*w}$ and $h_a l / v_{*a}$, containing the friction speeds appropriate for the two layers,

$$v_{*w} \equiv (\tau_o / \rho_w)^{1/2}, \quad v_{*a} \equiv (\tau_o / \rho_a)^{1/2},$$

where τ_o is the tangential stress at the surface and ρ_w and ρ_a are the densities of water and air. In terms of the resistance coefficient γ .

$$\tau_o \equiv \rho_a \gamma^2 v_a^2, \quad v_{*a} = \gamma v_a,$$

the comparable numbers become

$$\frac{h_w l}{v_{*w}} = \frac{h_w l}{v_a} \left(\frac{\rho_w}{\rho_a} \right)^{1/2} \frac{1}{\gamma}, \quad \frac{h_a l}{v_{*a}} = \frac{h_a l}{v_a} \frac{1}{\gamma}.$$

The coefficient γ varies somewhat; reasonable values are 0.04 for the ocean and 0.06 for Boston. As the ratio of densities is about 900, the resulting values of $h_w l / v_{*w}$ and $h_a l / v_{*a}$ are both about 0.24, so the number appears to take on a universal character. Rossby (1932), in terms of a

constant k , had concluded theoretically that the thickness of the turbulent layer h both in the ocean (his eq. 92) and in the atmosphere (his eqs. 167, 168) should be given by $h/v_* = 3k$. In his more detailed theory the factor 3 is replaced with a number roughly a tenth larger, and in the 1935 paper the value of k was concluded to be about 0.065 for both atmosphere and ocean.

A quantitative theory of the thermocline itself was attempted by Munk and Anderson (1948). Some of the assumptions are arbitrary, and the real basis of the phenomenon does not seem to me to have been elucidated, but the paper is noteworthy because it represents the only attack on this difficult problem.

(At the conference, Stommel pointed out that in addition to the two processes already mentioned, i.e., warming followed by cooling, and wind stirring, two further processes may be important in the genesis of thermoclines: One is that the presence of lateral boundaries in lakes requires closed circulations in one or more layers, which thereby tend to become isothermal and to be separated from one another by thermoclines. I may add that such circulations have been observed in models by Sandström["], 1908, and have been reported to occur in Lake Mead by C. P. Vetter, 1953. The other process forms the subject of Rossby's 1951 paper on the vertical concentration of momentum in ocean currents, a mechanism that tends to change a uniform density gradient into an internal stable layer.)

5. Description

Although there is a great deal of interesting published information on

temperature distribution in lakes, there is surprisingly little literature on thermoclines in the ocean. The brief treatment in Harvey's (1928) book is perhaps as extensive as any. Reid (1948) has suggested a formula to represent the vertical distribution of density in the equatorial thermocline.

A number of unclassified reports have been prepared that, although not concerned primarily with the thermocline, contain much information about it. One of the most interesting of these is F. C. Fuglister's "Completion report on the hydrography of the western Atlantic. No. 4. The hydrography of the northwestern Sargasso Sea," Woods Hole Oceanographic Institution, 1947.

Further information is contained in various classified reports. The most complete account of the oceanic thermocline that I have seen is in "The application of oceanography to subsurface warfare," Summary Technical Report of Division 6, NDRC, 1946 (restricted).

6. Heat Exchange

Thermoclines depend, some directly and others indirectly, on the heat exchange across the interface between water and atmosphere. This heat exchange, on which there is a large literature, is therefore of great importance in many considerations of the thermocline. Two especially significant papers will be mentioned.

Stommel and Woodcock (1951) have studied diurnal heat exchange in relation to temperature changes observed by bathythermograph from Atlantis while drifting.

Much new information on heat exchange is contained in the Lake Hefner report by Harbeck et al. (1952).

A comment may be made on one aspect. As the present interest is not in the effect on the atmosphere but in the effect on the water, to calculate separately the exchanges of sensible heat and latent heat is an indirect (if not, indeed, artificial) means of finding the total heat exchange by transport phenomena (i.e., by processes other than radiation.) Suppose the unit-area upward flux of sensible heat can be expressed as

$$Q_s = f(t_i - t_b),$$

where t_i is the temperature at the interface, t_b is the air temperature at a distance b (a few meters) above the interface, and f is a factor depending especially on b , on $t_i - t_b$, and on wind speed. The unit-area upward flux of enthalpy, or total unit-area flux of heat by transport phenomena, can then be expressed as

$$Q_t = f(t_{ei} - t_{eb}),$$

where t_{ei} and t_{eb} are the corresponding equivalent temperatures. The factor f has practically the same value as before, because the processes transporting vapor are nearly the same as those transporting sensible heat. The interfacial equivalent temperature t_{ei} is determined by the interfacial temperature t_i alone for a given salinity. The equivalent temperature t_{eb} is determined by the wet-bulb temperature t_{wb} alone for a given pressure. (The scale of the wet-bulb thermometer can be graduated to indicate equivalent temperature directly. See, for instance, Robitzsch, 1951.) The air temperature t_b thus does not appear explicitly,

although of course the stability indicated by $t_i - t_b$ affects the factor f.
(For further discussion, see Montgomery, 1950, section 7).

7. Internal waves

A thermocline provides a favorable environment for internal waves, which in turn distort the thermocline. While any general discussion of thermoclines would be incomplete without mention of the great importance of internal waves, no attempt is made in this summary to review the large literature.

8. Forecasting

One of the unpublished reports dealing with the forecasting of temperature and thermoclines is that by D. P. Martineau, "An objective method for forecasting the diurnal thermocline," Woods Hole Oceanographic Institution, reference no. 53-21, 1953.

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E. R. ANDERSON

Comments on: W. H. Munk and E. R. Anderson, 1948:

Notes on a theory of the thermocline. J. Mar. Res., 7, 276-295.

I think that paper has been distorted somewhat. You read it perhaps rather quickly. It was, of course, never meant to be a complete theory of the thermocline. It was meant only to indicate what four different things might do, or how they might act. If you go through it, you find that the depth of the mixed layer, in this paper, is a function of four things: wind speed, stability of the water, heat flux, and latitude.

Now if we can assume that the mixed layer is a function of these four things, we can get the results presented in this paper with certain restrictions on them which are rather unfortunate. A zero or positive heat flux was assumed. That implies, of course, condensation. Positive means that you are adding heat to the water. In other words, the whole energy budget gives zero or a net addition of heat to the water, not a taking out of heat. That of course limits it very strongly because most of the time that condition is not met. It was also assumed that this heat flux is independent of depth, which is a bad assumption. It is probably an exponential function of some sort. So if you keep those restrictions in mind, you get the paper in its proper perspective.

But what is left out then that seems important is convergence and convection, and also convergence and divergence, or advection. Austausch transfer of heat is ruled out by this. We are never getting cooling at the

surface -- therefore we are not getting convection.

These terms are all related to each other, not mutually independent. What this was to lead to was a list of things that seem to be important in considering the thermocline. Convection and advection are neglected and, I might add, so is the effect of internal waves. Now the paper showed only the effect of the four things that were listed at the beginning. In the two comparisons that were made, we attempted to define situations where this issue was met. Of course that is very difficult to do and such a situation probably never actually occurs. But in these two comparisons, as you recall, the computed depth of the mixed layer was about 0.42 times the actual observed depth which, to Munk and myself, indicated that the wind was important but was not the only important thing. It seemed, at the moment, that convection was equally as important and that if you ever wanted to get a complete theory that described it all you would have to consider these four things, plus at least this additional factor. If you used it in areas where there was strong convection, you would certainly have to consider the latter. These seem to me to be the main processes here, or those portions that must be considered now; it is something we can put on the board to start arguing about.

BERNHARD HAURWITZ

The thing with which I am struck, being primarily a meteorologist, is the similarity of the problem, in many respects, to meteorological problems. In the first place, I must say that I am very glad that the

oceanographers have to worry about some forecasting problems. Of course, we in meteorology have had to worry about that for quite a while. My happiness is tempered by the fact that unfortunately it again seems to be the meteorologists in the Navy who have to use the product. But at least this time, if it does not work, they can blame somebody outside their own particular field.

The other thing which comes to my mind is that here we evidently have something very similar to meteorology, inasmuch as in meteorology the layer of the air next to the surface -- it does not matter how high it should go, let us call it the planetary frictional layer -- is one of the most important layers, not only because we live there but because the exchange between the surface of the earth or the water and the higher atmosphere takes place through that layer. It is quite evident, on entirely general grounds, that the top layer of the ocean will play a similar role for the ocean as a whole because the interchange of properties from the one medium, the atmosphere, to the other one will take place in this layer. I am also reminded of meteorological problems when I see some of these diagrams sketched here which at first seem without rhyme or reason. My comment on those would be that if I had to do something with them I would first of all try to get some notions about what sort of types I can distinguish here. I will not try to do it now because I have not thought about it. And then I would try to see how I could explain them in terms of the various factors which are listed on the board.

Incidentally, I would like to say with respect to all these things which are on the board here, if you really would want to work with them you would

have to be careful -- even if we omit the controversial convection and replace it by something which might be better -- inasmuch as some of them here are really physical processes like convection or internal waves or heat flux, and others are really parameters. Of course you can use a physical process in order to account for the development of these various thermoclines. But on the other hand, you would have to use a parameter, say like the stability, in order to correlate it with the physical process. I think that is quite important to keep in mind. For instance you see things like this: we have the latitude in here as a factor. Now as Ray Montgomery explained yesterday, there may very well be a relation between the latitude and the depth of the thermocline, apparently via the Coriolis parameter. On the other hand, it is not entirely certain that this is so, that the reason for the latitude entering in is really the Coriolis parameter. It might come in via the heat flux because the heat received from the sun depends on the latitude too and, in fact, we might even find that the latitude enters into the whole system in two places.

If I had now decided that there are various types of thermoclines, preferably not too many, I would probably first try a purely statistical approach. That is, I would make a list such as this, or one similar to it, and then see which of these parameters seem to correlate well with the various types of thermoclines and which do not. However, I have my doubts whether that would lead me very far, because you can evidently think of lots of factors and you probably have not enough data to make a satisfactory correlation. So evidently what one would have to do very

soon -- and it is quite obvious that a lot of people have started to do that, fortunately -- would be to develop a physical theory of the thermocline, how it originates. Well, you will remember, some people have started on that and some have apparently even been successful in describing and forecasting the development of thermoclines. It seems to me that this would be the most fruitful approach to the whole problem, namely, to develop a theory, at first as simple as possible of course, and then see how it checks with the data. Then, as you find the theory does not work, you would either reject the whole theory or make it more elaborate. There is another advantage in that: once you have a prescribed notion or hypothesis or theory -- whichever name you want to give to it -- then you know what you actually want to look for in the ocean.

I think probably one of our troubles with these things has been that, with the observations as we have them now, an index had to be made more or less at random, that is, it was largely a purely data collecting job without any particular attempt of trying to either prove or disprove any theory. I remember this in particular because I tried to help one of the doctor's candidates who is now at Woods Hole and remember that, unfortunately, there are a lot of data collected but not in the form in which they can always be used, and of course it is now too late to go back out in the ocean at the same time and the same spot and see how it really was or what happened the day before or the day after. Therefore, I think that for purely practical purposes it is very good to have some hypothesis which not only shows you what to look for but which shows you when to make the measurements and for how long a period.

Finally, I would just like to say that even if we do not come up with a definite program of work where one does this and the other does that, I feel the meeting has been quite a success, if for no other reasons than that we have at least found out better than we could by correspondence what the other person is doing. I certainly do not feel that my time has been wasted even if I do not go back now with the idea that those points should be investigated and those are already solved.

T. F. MALONE

My knowledge of the thermocline is limited to what I have heard in the last few days. My viewpoint, for which I will not apologize, is of course meteorological. As such, probably I would be concerned more with the upper portion of the vertical temperature distribution, the part of the BT which might lie between orders of 10 and 10^3 feet. This seems to be most immediately related to the meteorological conditions and I think, considering the overall problem here, there is some hope for a contribution, meteorologically, to the operational problem. I do not want to violate the philosophy of this meeting by getting into the operational aspects, but as one whose background is forecasting I have a keen appreciation of the forecasting problem involved here, and I think that simply because certain meteorological aspects might be useful in the forecasting sense in no way lessens their fundamental scientific importance.

Now this picture is quite confused in my mind and perhaps the only way I can make any sense would be to oversimplify it. If we restrict

ourselves to the upper part of this vertical temperature distribution, we might go one step further and draw a line of distinction between what could be called the fine grain structure and the coarse grain structure there. The fine grain structure is going to be very complicated. I gather from conversation with Ernie Anderson about the very interesting work that he has done, that even introducing the meteorological factors there, such as solar radiation, is going to require a refinement which, if I understand Irving Hand's comments on the accuracy of the pyrheliometer, gets down to where we are crowding even that. Now, if we can take that over-simplification -- and we shall worry for the moment about the coarse grained structure of only the top of this BT which, as Dr. Eckart put it yesterday, does have a basic relationship to the permanent thermocline -- what can we do about it, or say about it? Well, there has been talk about forecasting this. It seems to me that before we can even think of forecasting this, we have to get a little better idea about the relationship between meteorological factors and the temperature distribution.

Eighteen years is a long time, but I think we have to go back that far to get the philosophy of approach which to me is most applicable now. The work that Montgomery and Rossby did seems to me to be directed along those lines. During those intervening years there have been enough data accumulated in an oceanographic sense, and certainly we have a lot more meteorological parameters with which to work, so that it seems to me the time is opportune to pick up the philosophy which, I believe, guided Montgomery and Rossby in that work some time ago. Now, what can we do about this relationship? I think there are two things that are important.

We need to get a better idea of the time scale and a better idea of the space scale.

There was some discussion yesterday about the response of the temperature distribution to meteorological factors and I am left with a great deal of uncertainty about what time scale is involved here. It is certainly not a day, I would gather, but maybe five days, maybe thirty days. I would pick those two periods because the meteorologists have a little information about the broad scale patterns and, whether or not they would work out, they would certainly be the first things at which I would grasp. So it seems to me that it is quite important here to establish this time scale. What meteorological interval do we want to use to relate to the particular BT?

We also need a space scale. Do we need to work with only a small area or do we need to work with a broad circulation pattern? The meteorologists have been enlarging their horizons during the past decade or so, so that we are now inclined to look at weather not as that which can be represented by a map of the U. S. but by a global map. For your problem here in the North Atlantic these very large scale weather processes may be important. I do not think that we really know what scale is involved or how far out you have to go meteorologically to relate to what is happening right here. That seems to me to be a thing that needs to be looked into. Possibly John Freeman's analysis of the BT and the ocean weather ship in relation to the conditions right around it may need to be enlarged. He probably has this in mind. It would be my thought that here the area has been too severely delimited. So those two things then need to be established. They are essentially

important in any forecasting approach and I think they would also be of extreme importance in working out the basic scientific relationship.

Now how can you go about this? I was intrigued by a sort of hint that Giff Ewing mentioned almost as an aside on Monday, that you might be able to work with some kind of a normal temperature distribution or average temperature distribution and then departures from that. That seems to me to hold the nucleus of a fairly good approach. In that connection I would like to emphasize to a group consisting primarily of oceanographers that when we talk about a climatic mean we have to be very careful. If you talk about the normal pressure distribution for the month of January, in one sense that is fictitious because it is something you do not actually find in a given instance very often, and when you start studying the distribution of BT's over the North Atlantic you probably cannot look at them in terms of a normal circulation map for a given month. I think that was brought out in John Freeman's diagram showing how the curve changes from one year to another.

If you want to have one suggestion as to how this might be approached -- it probably would not work, but it is the only one that occurs to me -- it seems to me that it might be of some use to establish some types, as Bernhard Haurwitz suggested here, and then worry about departures of these types in terms of departures of the circulation pattern, the temperature distribution, and the insolation, from normal. As a means of doing that I would like to mention just briefly an approach that we are somewhat interested in, which we have taken from George Wadsworth who is working

with us on this and which seems to me to be rather a promising way of studying departures from normal circulation patterns. Essentially what it involves for any meteorological parameter is to plot the value of the parameter itself, which is, in fact, the mean of the month, the five day period, or whatever period you are working with, and dividing that by the standard deviation. You can compare charts with this standardized unit -- pressure, height, temperature, or practically anything -- and the particular virtue it possesses is that it adjusts for the standard deviation. In other words, a big departure at high latitudes where the variability is large is then brought down to a comparable scale with a small departure down in the southern latitudes where the variability is very small. And I think that in studying fluctuations or aberrations of the circulation in lower latitudes, or the temperature distribution, this would be a very good way to adjust for the difference in the variability of the meteorological parameters. If you assume a normal distribution you can express it directly in terms of probability and, if you get discouraged with some of the statistical complications, you can avoid this course and get the same thing by using actual probability distribution. It would seem to me then, this characteristic that Wadsworth has evolved might be a rather interesting way to prepare charts such as these for any time interval -- charts of temperature, charts of the circulation, charts of insolation.

I think something can be done with insolation. Ernie Anderson discouraged me quite a bit by telling me how precisely you have to know the incoming solar radiation. We have done a little work in expressing solar

radiation in terms of the theoretical value which would come in, a value which can be obtained very handily by means of a little device which Libby-Owens-Ford Co. puts out. We give the values in terms of, say, the average percentage of sunshine per month with an additional meteorological parameter in there for the number of days, the number of clear, partly cloudy or cloudy days. I am still enthusiastic enough about this to think that you might be able to use the approach -- this method of computation -- to get the proper kind of solar radiation data to plug into these time-averaged maps and provide some worthwhile information, although I am rather pessimistic about getting the fine grained structure.

I have one final comment and hope I will be pardoned for mentioning operational applications again. If another conference is planned in which the operational problem will be the central theme, it would be very helpful to this admittedly extremely difficult problem to tell us how precisely we must know these things. I arbitrarily divided these into a coarse structure and a fine structure simply because I do not know, and no one has been able to tell me, how accurately you are going to have to know the vertical temperature distribution in this upper part of the ocean in order for it to be useful in an operational sense. Scientifically, we can go ahead and investigate this and learn a lot of things. Operationally, we may be going way beyond what is needed, or it may be that our best efforts fall far short of what is needed and are useless. It seems to me that the operational people should make a very strong effort to provide us with intelligent guesses.

CARL BEHRENS

A truly enormous amount of effort is going into that most important application of oceanography, the transmission of sound. Having had the great privilege of working for Dr. Eckart at San Diego for two years, I have always since that time been convinced that any real progress we can expect in using the ocean as a medium for transmitting sound lies with the oceanographer rather than with the gadgeteer, the man who wants to put another 10 db of power into the water and put another 100 tubes into the gear. So the remarks that I would make would be in the form of a plea to the oceanographers to keep the enormous extent of this effort in mind as they carry out their legitimate scientific function.

The picture has changed quite radically since the days of the war, in connection with this forecasting problem and the thermocline. I do not know how representative I was, but I found it perfectly all right during the war to pretty well forget about this thermocline and to concentrate on the upper 200 feet of the ocean, and also to restrict myself to a rather small area. We all know these conditions have been entirely changed; we now have to worry about the whole ocean from surface to bottom, and we have to take in a lot of area. Hence the work that has been discussed here these days will, from this very practical standpoint, become all important.

As far as forecasting is concerned, and its use to the Operations Evaluation Group, everything that can be found out about it is grist to our mill. Though I have not thought about it enough to indicate just what specific

use one can make of the particular kind of forecasting or prediction, I think it is necessary that the oceanographer guide quite closely the efforts of the man connected with the transmission of sound in the ocean. I know a lot of that is going on now.

I have one more point, which I am very hesitant to make because I cannot speak on it very well, and that is that averaging procedures as they are connected at all with oceanographic data -- in connection again with the transmission of sound -- be subjected to pretty rigorous examination. I have a feeling that oceanographers, when dealing with this subject of the transmission of sound, probably can do a lot of practical good that way. I mention this because occasionally, in our group, we raise an eyebrow a little bit at some of the averaging procedures that do go on in the work on the transmission of sound.

R. B. MONTGOMERY

I would like to follow up a little further Bernhard Haurwitz's suggestion that there are comparable matters in the atmosphere which can be related to the oceanic problems to advantage. I want to mention specifically what has not been mentioned before to any extent, that the radio propagation in the atmosphere is very closely related in a number of ways to sonar in the ocean, and that it has been studied and there have been attempts to forecast propagation conditions in the atmosphere. The meteorology and the oceanography are almost exactly parallel in the two. You have the thermocline in the ocean, you have inversions in the atmosphere, and

you have mixed layers and ducts and all these things which are exactly comparable.

Not only that, but where we started here was with the assignment to the aerologists in the Navy to undertake the problem of forecasting sonar conditions. I believe they are also in the same manner concerned, or could be concerned, with the propagation of radio waves in the atmosphere, and it would be to great advantage in several ways if the people concerned with sonar propagation would acquaint themselves, to some extent at least, with the work that has been done on radio propagation. There have been a lot of publications on this subject, and one I would like to mention especially is the book on radio propagation put out by the MIT Radiation Laboratory, Volume XIII of their series published by McGraw-Hill.

J. C. FREEMAN

It seems to me that here we are studying the ocean temperature structure, that we described it, that we discussed, to a certain extent, mechanisms of how the temperature structure is brought about and that we discussed forecasts. Concerning space description of the temperature structure in the horizontal and the vertical -- and if you think of it primarily as steady state space description -- I would say that Fritz Fuglister is tying it up pretty tightly. I think the time description of the thermal structure is pretty weak and that this is where we should concentrate, particularly on non-cyclic time descriptions.

Under mechanisms we have the various mechanisms of mixing and stirring. I think that we should concentrate on those. Then we have other mechanisms: salt flux, heat flux, surface and internal waves, balanced and unbalanced currents, advection, radiation, evaporation, gravitational stability and earth's rotation. All of these things should enter into mechanisms on the temperature structure. As I say, it seems to me that our concentration in future studies should be on mixing and stirring since we already know so much more about most of these other mechanisms and how they would enter in. If we are going to look for anything new it should be in mixing and stirring.

In the making of forecasts I would use the military language and say, strategic or relatively long range forecasts -- sometimes you can just list a description and make it into a strategic forecast -- and tactical forecasts. And now I shall repeat my remarks of yesterday, that I am most hopeful of getting some way of describing the really small scale things like the little thermocline that Jack Schule told us about, or the diurnal effect. I am hopeful of getting some way of describing those things so that our forecasts would be giving our best estimate of what a description of those things for today would be.

F. C. FUGLISTER

I believe that we are going to get someplace in this forecasting, but maybe our immediate object should be to pick out areas in the ocean where we are going to do this. I have a feeling that there are certain areas in the ocean where we are going to be able to forecast conditions -- simply because

when I go out there I feel already that I know what I am going to run into -- and that there are other areas where we are going to have a great deal more difficulty. Of course, when I say that I know what the conditions are going to be like in one of these areas, it does not mean that I know what the internal waves are going to be. I do not know how we are ever going to forecast internal waves.

There is another type of wave that I would like to mention -- something I might call an internal storm wave. We can run along in the Sargasso Sea, making our temperature-depth observations every half hour or every five minutes, and find a little seasonal thermocline whose depth seems to fit in to just about what we would expect in that area at that time of year and which wiggles up and down slightly because of internal waves. But once in a while, if we just let our BT down to 450 feet, we find that this seasonal thermocline introduces some new isotherms at the bottom, as when we get into cold water. On at least half a dozen occasions we have been fortunate to have deep station data in an area like that, and these indicated this kind of structure -- which we call eddies -- which definitely changes the whole picture and of which I think as storms in the ocean. These may come from a frontal region like the Gulf Stream.

Now, in the case of the classical example, the Sargasso Sea, I believe we can predict pretty well what the temperature-depth structure for the various times of the year would be and we may be able to do it even better when we use synoptic or five day mean weather data. While we shall be able to predict normal temperature structure, I do not think we shall be able to

predict these abnormal conditions. Therefore, when a person is making observations out there and studying the variations, seasonal or otherwise, in the seasonal thermocline, he should be sure that he is in our typical ocean with the conditions we expect to find in the Sargasso Sea, and that he is not dealing with a situation where one of these internal storms may be passing through the area.

I think a person may make internal wave measurements and get these high amplitudes on occasion simply because of one of these eddies, or because they happen to be too close to the front when making the observations and are studying the large scale current in the area.

G. C. EWING

Could we make this as a concrete suggestion, carrying out Fritz Fuglister's idea that the study should be made in some particular area? If you are going to try to forecast these things operationally, why not try to do this in an area where operations are being conducted? Now the ones that come to my mind offhand are those in the neighborhood of USNUSL, Portsmouth, Key West, and San Diego. Why would it not be a good idea to set up a pilot experiment somewhere where sound vessels, perhaps attached to a sound school, go out once or twice a week on an operation and try to see whether, as a practical matter, you can develop a forecasting technique in this standard area?

D. W. PRITCHARD

We have had sufficient proponents for the necessity of giving early practical pressures to the operational problem of getting some improved answers on thermocline forecasting. I would like to move ahead and project what we might look forward to in perhaps the far distant future. Here I disagree to some extent with Bernhard Haurwitz in regard to how much of a coverage might be necessary for the ultimate forecast of the thermal structure, even in the lower layers of the atmosphere. I have been engaged in operational forecasting in meteorology too, and would not be very satisfied to use only local conditions to forecast temperature structure in the atmosphere. I think that ultimately we are going to have to use more than local conditions to forecast thermal structure in the sea, and that the thermal structure is intimately tied up with the hydrodynamic solutions of the complete motion in the ocean as well as with the local mixing phenomena.

I foresee the necessity of someone -- or perhaps a limited group of people -- using whatever factors appear to be necessary from our knowledge of the atmosphere, constructing synoptic charts and setting up the techniques of taking those atmospheric data, using our knowledge of the general circulation and modifying it by applying the synoptic conditions, and trying to get at least the formalized solutions to the motion and the resulting thermal structure. This, of course, involves the overall solution of the hydrodynamics of the ocean, but I think it is something we should look forward to, that some group capable of doing this should undertake it.

The formal solution would probably take a long time and, in itself, might not be the answer, but it might form the framework to which the local solutions and the individual studies on different aspects of this list, including the things that we do not know about, could be plugged in.

W. V. R. MALKUS

Dr. Haurwitz said earlier, and I believe it is quite appropriate, that one needs a preconceived model for a measurement program to be particularly successful. The measurement program will test the validity of the model and change the nature of the preconceptions or make new ones. In the situations we are dealing with here, with the thermocline, a tentative list might add something if we adhere to it. Momentum flux, I would say, is really up in this region included in the Munk - Anderson paper. This list is supposedly more or less conclusive of the kind of parameters which influence the thermocline. Yet also, as Dr. Haurwitz pointed out, a number of these things are not, in their very nature, parameters. Latitude, maybe, stability defined parametrically, but heat flux, convection, advection, and momentum transport are processes.

Now if we had been successful in parameterizing the process -- and we usually seek to do this -- to contain all of the complexity of the single physical process and mechanism in one simple set of symbols, as Munk and Anderson sought to do by their use of the Austausch coefficients, many of us would be quite content. But when these parameterizations of processes fail, when we do not find that our choice of parameters describe the processes

adequately, we must then look more carefully at the process itself.

Here we have a number of inter-acting processes. Heat flux in this case is due to momentum transport occurring simultaneously. Wind speed may influence the formation of waves and wave mixing in the very upper regions. Advection is a large scale process, and we might have to study large scale physical oceanography to know how the advection influences the thermocline in local regions. Convection also is a complicated process of its own.

I find it necessary to attack individual processes and try to understand them, to see if their parameterization is justified, before I could combine them and test the joint validity of many of them. If it is possible to test the individual validity of single assumptions then naturally one should go on to test the joint validity of many assumptions. Hence, our work has been more or less directed toward investigation of single processes, in the case I spoke of yesterday, the convection process. That may play an important role in the thermocline. If we understand how mixing depends upon gradients, that may be some help. Next perhaps on the program should be simple momentum flux and how it depends upon boundary parameters and then, perhaps, we can run the two together. At Woods Hole we are going to try to make a series of isolated models which, as I mentioned, will take years before they come together in a complicated situation like this. But it is to be hoped that long before these years have passed, a successful parameterization will have permitted a partial prediction of thermoclines.

CARL ECKART

About all I have really to say is that there is something to be said for rational mechanics. Rational mechanics in our field is not called that, it is called the hydrodynamics of incompressible, homogeneous flow. That field has not contributed much to oceanography; it has contributed a great deal to other things. It fits in, in a rather intangible way, by giving people something to shoot at, something to knock.

We have nothing similar for oceanography. The so-called useful hydrodynamics of incompressible homogeneous fluids is not the type of rational mechanics needed by oceanographers. I think what one needs is the rational mechanics of a stratified fluid, which is worked out with some care and logic and no regard to the immediate application, no fear of what is destroyed because one neglects convection or heat flux or something else. By stratification I mean two things: the density gradient, the lines of constant density which we shall probably take as horizontal, and the lines of constant velocity, also horizontal, but with shear. If one works out a simplified theory of such motion, perhaps we will ultimately know the very complex equations.

I do not think our situation is much more complex than the situation in regard to atomic spectra which also leaned heavily on rational mechanics in its early stages. The history of that is that the sodium B line was discovered about 1814. The discovery was promptly forgotten and was rediscovered in the '30's. A vast amount of data, completely uncoordinated,

was accumulated during the 19th century and then in the early years of this century people began trying to interpret that with the aid of rational mechanics. The final stage, when one departed from the useless mechanics and developed the mechanics which would actually fit the data, occurred rather suddenly under a certain amount of explosive bile and pretty much overshadowed the tremendously long, century-long, period of development which preceded that.

I have a feeling that perhaps trying to take our empirical laws about these various phenomena, Austausch and a few others, and then putting them all together, as Munk and Anderson did in a bold attempt, is useful and has not been by any means exhausted. But instead of trying to do that, perhaps what we first need is not so much a model as a rational foundation which will take out certain field of the problem to see how far those ideas will carry and what new ones will have to be deduced.

G. G. LILI.

The chief value of this sort of meeting to people like myself is that it furnishes a mechanism by which we can keep ourselves refreshed as to how science and scientists work. In the kind of job I have, if I lose sight of this kind of work it could be quite damaging to science. This philosophy runs pretty much through the Office of Naval Research, and I feel our organization can take some pride in the way it has handled the program. I think this is so chiefly because we get a chance to go out and mingle with

the scientists in meetings like this, and I should like to thank you all for coming and helping to keep me refreshed.

POST-CONFERENCE ADDITIONS

HENRY STOMMEL

(Letter to thermocline conferees, dated 28 May 1953)

It seems to me that insofar as an immediate forecast procedure, particularly of the diurnal thermocline, is concerned, the prospects looked fairly glum when the meeting closed. On the bus trip to the station, however, John Knauss, Giff Ewing, John Freeman and I all examined the data shown in the accompanying figure (copied from: Henry Stommel and A. H. Woodcock, 1951: Diurnal heating of the surface of the Gulf of Mexico in the spring of 1942. Trans. Amer. Geophys. Union, 32, 565-571).

Upon examination of these data with the very limited objective of making a 24 hour forecast of the diurnal thermocline, the results looked so encouraging and so suggestive of the kind of data that we need to accumulate that I wish to communicate them to you in this letter.

Martineau has prepared a WHOI report, Reference No. 53-21, entitled "An Objective Method for Forecasting the Diurnal Thermocline" consisting of a set of rigid rules for forecasting the difference of temperature at the surface and 50 feet. I did not see this report before the conference and have just had a quick look at it before writing to you. It is my opinion that one can make considerably better forecasts on the 24 hour basis if he abandons Martineau's rigid objective scheme and simply goes whole-hog subjective and uses plain, everyday horse sense. At any rate, I would like to show you on the basis of the very sparse amount of data

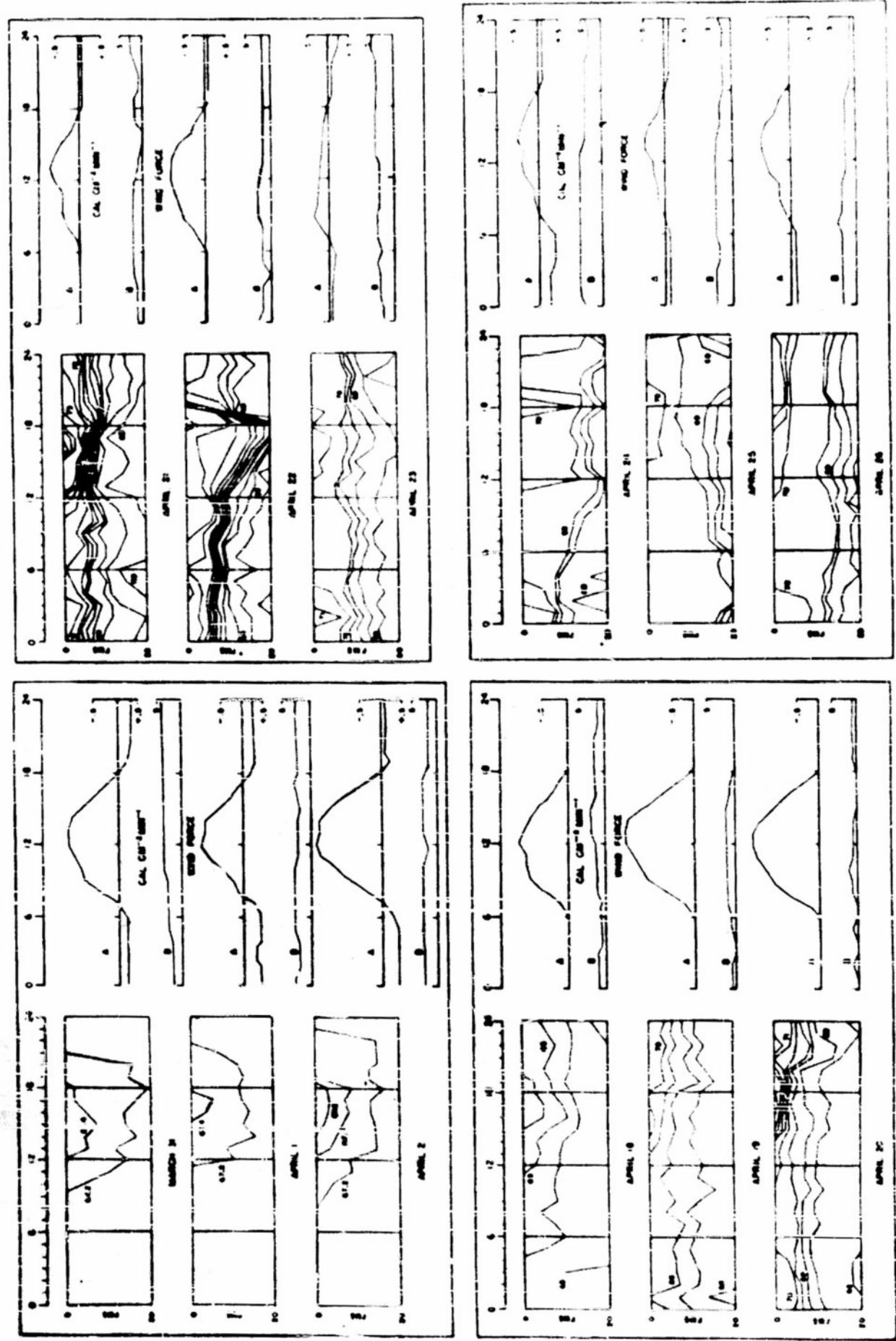


FIG. 2.-Selected cases of diurnal heating; the time-depth profiles drawn from the hourly bathythermograms are drawn on the left; the contour interval is not the same in all cases: 0.2°F for March 31, April 1 and 2, but 0.5°F for the other days; the computed net heat flux positive upward at the surface is shown plotted against time of day on the right in the curves labelled A; the observed wind force as a function of time in the curves labelled B.

which I have, just what I mean by this very unscientific sounding remark.

Now suppose we look at the figure, at the diagram of temperature against time for March 31. We see a diurnal thermocline develop with 0.2°F isotherms developing during the afternoon, extending to 20 fathoms to the deepest. To the right of this temperature-time graph are the accompanying computed heat flux and wind force diagrams plotted against time. Now the first drastic exercise of horse sense I propose is to look at only the most general features of the heat flux diagram. Anderson already showed us at the conference that the formulas used in computing the heat flux are very shaky, so I propose to suggest that we regard this heat flux curve as simply an indication of the amount of insolation during the day. Between 0600 and 1800 this curve is predominantly influenced by the percent of cloud cover and so we may interpret it simply as the percent of clear sky. I propose therefore in this initial subjective method to forget about all processes that transfer heat across the surface except the sun. The wind we will regard as a mixing influence and we will treat this influence in the forecasts in a purely subjective fashion. All right, now we will suppose that the meteorologist has given us a forecast of the percent of clear sky and wind force during the next day, April 1. We will suppose that the forecast that he gave was a correct one just like the actual observed percentage of clear sky and wind force as actually observed on April 1. Well, my horse sense tells me that the amount of heat put into the water during April 1 is about the same as on March 31 and that the wind, if anything, is slightly less, that is, averaging force 3 rather than 4. Therefore, as

long as I stay in this one position I would expect the diurnal thermocline to be almost identical to that of March 31, and if I were on board the boat I would make the following forecast:

Subjective forecast for April 1. The diurnal thermocline will be almost identical to that of March 31. Perhaps the only difference will be that it will be slightly shallower on account of the weaker winds and may persist for an hour or two later in the evening for the same reason.

Verification. We see that this forecast is really verified in almost complete detail.

Of course, in making this forecast I already know what the real answer is, but after the conference I went through this procedure with three individuals and concealed the actual thermocline data from these people until after they had made their forecast. I am trying, in making these forecasts, to recall the kind of forecast which they actually made. As you will see, things don't always turn out quite as nicely as this.

All right, we will go ahead now and try to make a forecast for the next day on the basis of weather information for April 2. Looking at this curve, I think we could make the following forecast:

Subjective forecast for April 2. There appears to be about 50% more sunlight forecast for April 2 than for either of the previous two days. For this reason I forecast 3 isotherms rather than 2 isotherms in the diurnal thermocline. The wind falls off to force 2 after sunset and for this reason I forecast that the thermocline persists to even later in the evening than either of the two days before. I cannot predict just how long it persists, however.

Verification. As will be seen, 3 isotherms actually do occur on April 2 and the persistence is actually longer than either of the other two days.

There is now a gap in the data and I think it would be a very brave man, indeed, who would try and forecast the thermal structure for the next date on

which we have data, April 18, so let us suppose that we now have gone out in the boat again and do not try to make any forecast for April 18. We will simply use the data for April 18 as a base for making the next 24 hour forecast. What is more, you will notice that the thermocline developed on April 18 is about twice as strong as that developed at the first of the month and is somewhat shallower even though the sunlight is less and the wind not very different from April 2. You will also notice that the thermocline persists late into the evening. I cannot account for this, but I don't think that it is at all possible by the subjective methods to do so. We simply note these facts and use them as a basis for making our prediction for the next day.

Well, now let us look at the heat flux and wind data for the next day.

Subjective forecast for April 19. Since there is about 50% more sunlight I forecast 5 isotherms coming into the diagram during the afternoon and penetrating to about 10 fathoms. The wind drops off to calm right after the sun sets, therefore I predict persistence of this thermocline into the evening.

Verification. The heating of the surface does not appear to be nearly as strong as predicted. I cannot account for this. The thermal structure which is produced, however, does most certainly persist into the evening.

Subjective forecast for April 20. Looking at the forecast for heat flux and wind data for April 20 I say gentle winds during the first quarter of the day bring perhaps 1 isotherm to the surface. Because of overestimate of diurnal heating in the previous day's forecast, I now estimate 4 isotherms coming into the surface, fairly shallow, and during the evening several of these coming to the surface on account of light winds.

Verification. Cooling in first quarter verified. Heating during second and third quarters somewhat more intense than predicted. Also the fact that several of these isotherms come to the surface during the evening is verified.

We will note in all these cases of April 18, 19 and 20 that the thermal

structure below the depth of the diurnal thermocline is persisting from day to day whereas from March 31 to April 2 this was not the case. It is quite obvious, therefore, that any objective method which does not take into account the already existing thermal structure in the water is likely to give a very poor picture of what actually occurs. Well, now let us proceed to a forecast of April 21.

Subjective forecast for April 21. Less heat and more wind are forecast, therefore 2 isotherms should come to the surface during the first quarter, about 4 isotherms should enter the water during the afternoon and 3 leave the surface during the night.

Verification. The forecast is practically entirely verified except for the fact that from 1500 to 1800 three additional isotherms enter and quickly leave the surface for a very short time.

Subjective forecast for April 22. Since the sun and wind are much the same, I forecast similar structure to that observed on April 21.

Verification. This appears to be true for the first two quarters. The third and fourth quarters, however, are completely wrong. How do we account for this? Up until this time the ship had been slowly drifting and during the last quarter of April 22 the ship was shifted in position during several hours of steaming. The violent changes in the last quarter of April 22 are therefore due to advection. This is a measure of how heterogeneous a supposedly homogeneous body of water can be. Beware, our study so far neglects the fearful probabilities of what advection can do to a forecast.

Subjective forecast for April 23. Since we have moved to another body of water I would be very hesitant to make a forecast for April 23. I should suppose that the deeper thermal structure below 10 fathoms should be more or less the same and that because there is almost complete cloudiness and wind force of from 3 to 4 that the diurnal thermocline would be very poorly developed.

Verification. These very vague forecasts seem to be verified.

Subjective forecast for April 24. We see from the forecast weather data that there will be twice as much heat put into the water during the day of April 24 and that the winds remain high all morning and late afternoon. I would therefore forecast that 2 isotherms come

to the surface during the first quarter and that the lower isotherms be depressed. Since 1 isotherm entered the surface during the afternoon of April 23 and there is twice as much heat, I predict that this thermal structure be somewhat deeper, perhaps 10 fathoms.

Verification. This qualitative forecast is almost completely verified. The only puzzling feature is the entrance of an additional isotherm very late in the day. I cannot account for this.

Subjective forecast for April 25. The winds remain fairly high all morning and I forecast the persistence of the deep mixed layer to about 15 fathoms during the first two quarters. Since the amount of sunlight is less, I predict only 1 isotherm coming into the surface and leaving it again shortly after sunset.

Verification. The forecast seems to be correct except for the fact that the deep thermal structure rises for the first three-fourths of the day to about 10 fathoms. This is a surprise. I think it must be accounted for by some more general forecasting technique such as that of John Freeman, in which he considers the effect of the local wind curl on the depth changes of the seasonal thermocline. It will be noticed that this deeper thermal structure which rises during the day is the beginning of the seasonal thermocline and no longer should be confused with the diurnal thermocline. Almost exactly at midnight the 70° isotherm enters the surface again. I cannot account for this.

Subjective forecast for April 26. More sunlight, diminishing wind. I therefore forecast persistence of the deep thermal structure throughout the day and 3 isotherms entering during the afternoon. Because the winds are force 1 during the evening 2 of these persist and only 1 comes to the surface.

Verification. Perfect.

I have made an honest attempt not to fool myself in this game into making better forecasts than I would be able to make were I not to know what the actual course of events would be. Nevertheless, I may have entered a certain bias anyway. What you will notice, however, is that the application of simple horse sense to this prediction problem is promising. WHERE DO WE GO FROM HERE?

I can only suggest that we need more data of this very kind. We

need hourly BT's (SHALLOW BT's) made in one body of water for many days with accompanying cloud cover and wind force data. We then should get some practicing aerologists together and after introducing them to the simple ideas using the data which I already have used here, we can then see how they do on further more extensive series of data from other areas as well. People could play musical instruments before there was a theory of acoustics. I think people can make crude subjective forecasts before we get a rational theory of the thermocline. This does not mean that it would not be nice to have a rational theory of the thermocline. It just means that we probably can make some practical progress in the 24 hour forecast of the diurnal thermocline without such a rational approach.

C. O'D ISELIN

(From letter to M. J. Pollak, dated 8 June 1953)

If I had been at the Thermocline Conference I would have been inclined to be braver about my ability to predict a thermocline than most people were. To what Hank Stommel has written I would like to add the following:

1. You have ample means of knowing surface temperature and temperature at 200 meters, which does not change seasonally. You also know something about layer depth. Thus you have three points on a curve whose shape changes in a characteristic manner seasonally. Except near the edges of a current, the gross features of the temperature-depth curve seem easily predictable.

2. Diurnal warming is only difficult to predict in the early spring in situations of moderate winds and partial cloud cover. At other times of the year and under more extreme conditions I would bet that I could be 90% correct.

3. Don Martineau's technical report on diurnal warming seems to me to be too conservative as far as verification is concerned. His sample of data was heavily weighted with borderline situations.

G. C. EWING

(From letter to Henry Stommel, dated 17 June 1953)

Your letter of May 28 addressed to the thermocline conferees has inspired our naval research reserve group to set themselves up as a guinea pig group to see whether we can successfully carry out the suggestion made in your last paragraph. This is not an ideal arrangement as it would be better to try the plan out as you suggest with practising aerologists. If such an official group comes to life, we will fold up and blow away but, in the meantime, perhaps this is one of those twenty-five cent experiments that Harold Jeffreys has used so successfully. The group contains about sixteen people and some are on the staff at Scripps; some at NEL; one is a professor of mechanical engineering at State College; one, a stockbroker who is taking a correspondence course in aerology; and two are ex-World War II Navy Aerologists. So perhaps they constitute a fair sample of the Navy population which will have to undertake this work. Our plan as I see it at the moment is to set up an informal school to go over

some of the points raised in the recent conference, with the hope of giving them a rudimentary understanding of the problem. Next, we will try to find some suitable data to cut our teeth on and, finally we will break up into teams of two, each of which will undertake to make a forecast one day a week in some sort of collaboration with one of the local Navy Activities who would naturally be interested. Such an activity might be a local submarine squadron or the Navy Sonar School.

Admittedly, so ambitious a program openly invites disaster if it is presented as a scientific attack on the problem, but we have no such fancy pretensions. We are just going to try to uncover what will happen if a group of not-too-well grounded Naval officers, with some technical training, try to carry out the new Naval regulation. To be sure, aerologists probably will be better trained and, therefore, better able to carry out this function. But, on the other hand, they will be on moving ships for the most part and, therefore, will have difficulties to surmount that we will not.

At the moment our biggest obstacle seems to be finding suitable data to work on. Naturally, we think of the weather ships, but this would require getting one of them equipped with a shallow BT and arranging for having these sent to us, all of which would take some time. Also, we would like to deal with the local area as soon as possible. I think that this weather ship program should be undertaken, but that perhaps we can accomplish something while it is getting underway. We have at Scripps two anchor stations of three or four days' duration, which may also help us in the meantime.

However, I am going to try to find a way of getting two BT's a day taken in nearby waters where we will have facility in obtaining the kind of data that we want and modifying the specifications as we go along. If we could get one at 0800 and another at 1600 daily, these would represent, it seems to me, the extreme conditions to be expected during the day and would be the most useful ones to forecast.

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